EXHIBIT C

IN THE UNITED STATES DISTRICT COURT
FOR THE NORTHERN DISTRICT OF OKLAHOMA

State of Oklahoma,	)	
	)	
Plaintiffs,	)	Case No.: 4:05-cv-00329-GKF-PJC
	)	
v.	)	
	)	
Tyson Foods, Inc., et. al.	)	
	)	
Defendants.	)	
	)	

DECLARATION OF BARBARA KANNINEN, Ph.D.

I, Barbara Kanninen, Ph.D., hereby state as follows:

1.   I am currently a freelance editor and consultant with a focus on the valuation of

environmental resources.

2.   I received my Ph.D. from the University of California at Berkeley in Agricultural

and Resource Economics in 1991.

3.   As a Principal Investigator with the Hubert H. Humphrey Institute of Public Affairs

at the University of Minnesota, I conducted research to improve the design of stated-

choice experiments, including contingent valuation. As a Senior Economist at the

Damage Assessment Center at the National Oceanic and Atmospheric

Administration, I assisted in evaluating regulations guiding natural resource damage

assessment. As a Gilbert F. White Fellow at Resources for the Future, I conducted

benefit-cost analyses and studied econometric issues associated with stated-choice

studies and contingent valuation. As a freelance editor, I edited and published an

academic book on conducting and evaluating stated-choice studies, including

contingent valuation, for the purpose of environmental valuation. As a consultant, I

have designed stated-choice studies and analyzed data for the purpose of environmental valuation for private-sector and government clients.

4.   I am one of the authors of the State of Oklahoma's expert report entitled "Natural Resource Damages Associated with Aesthetic and Ecosystem Injuries to Oklahoma's Illinois River System and Tenkiller Lake – Expert Report for State of Oklahoma, in Case No. 05-CV-0329-GKF-SAJ" (hereinafter, "CV Report").

5.   I have received and read the expert report of William H. Desvousges, Ph.D. and Gordon C. Rausser, Ph.D., dated March 31, 2009 (hereinafter, "D/R Report").

DATA ERRORS COMMITTED IN CHAPTER 2 REGRESSION MODEL

6.   In the D/R Report, Desvousges and Rausser present a regression model that estimates the effect of water clarity on lake visitation at U.S. Army Corps of Engineers ("COE") lakes in Oklahoma. To represent water quality at each lake, Desvousges and Rausser use a variable called "meanclarity," which is a measure of the level of water clarity at each lake (D/R Report, p. 17).

7.   Desvousges and Rausser claim that their regression obtains a statistically insignificant coefficient on the "meanclarity" variable. A statistically insignificant coefficient means that the researcher cannot reject the null hypothesis that the independent variable in the model ("meanclarity") has no effect on the dependent variable (lake visitation). In other words, Desvousges and Rausser claim to find no statistical support for the idea that water clarity affects lake visitation. This implies, according to them, that "aggregate visitation for the COE sites for the years 2000 to 2007 was not impacted by variation in water quality, as measured by water clarity levels." (D/R Report, p. 18.)

8.   To check their claims, I and an associate economist at Stratus Consulting, Eric

Horsch, who has a Master of Science degree in Agricultural and Applied Economics

from the University of Wisconsin-Madison, re-ran the Desvousges-Rausser

regression model using the computer code and dataset they provided in their

considered materials (DesVousges-Rausser002865-visitmodel.log; DesVousges-

Rausser002861-OKvisitation.dta).

9.   In addition, Mr. Horsch and Scott Weicksel, an associate economist at Stratus

Consulting who has a Bachelor of Arts degree in Economics from the University of

Michigan, cross-checked the dataset that Desvousges and Rausser used to estimate

their regression model with the original sources. Mr. Horsch and Mr. Weicksel

discovered three errors in the dataset: two coding errors and one error of omission.

These errors are described below.

10.   Mr. Horsch and I re-estimated the Desvousges-Rausser model correcting for each of

these errors separately (without simultaneously correcting for the others), as well as

for every possible combination of the errors, including all three simultaneously. In

each case, as described below, "meanclarity," the variable that Desvousges and

Rausser rely upon to represent water clarity, is found to be positive and statistically

significant. This contradicts the claim that Desvousges and Rausser make that the

variable is insignificant and that, therefore, water clarity does not significantly

predict lake visitation. In fact, the water clarity variable in the Desvousges-Rausser

model is a significant predictor of lake visitation.

11.   One coding error in the Desvousges-Rausser model is in the 2007 visitation data for

Tenkiller Lake. When they ran their regression, Desvousges and Rausser used a

value of 294,047 for visitation at Tenkiller Lake in 2007. The actual number is 2,924,047 (Desvousges-Rausser002862-Lake data.xls, sheet1, "Visitation" tab). When we corrected for this error (and only this error), and re-estimated the model, we found "meanclarity" to be positive and significant: $\beta_{meanclarity} = 0.004$, t-stat = 2.25.

12. $\beta_{meanclarity}$ refers to the coefficient estimate on the "meanclarity" variable; "t-stat" refers to the t-statistic, which tests the null hypothesis that $\beta_{meanclarity}$ has no effect on lake visitation. When the t-stat is over the critical value of 1.96, the coefficient estimate is said to be "statistically significant" at the 5% level. The above results, therefore, show that the variable "meanclarity" is, in fact, positive and statistically significant.

13.   Another coding error in the Desvousges-Rausser model is in the lake depth variable at Fort Supply Lake. The Fort Supply Lake depth was mis-coded to be 0 feet, instead of 2,004 feet, as is provided by the COE, Tulsa District, website (http://www.swt-wc.usace.army.mil/FSUP.lakepage.html).[1] When we corrected for this error (leaving the other errors uncorrected) and re-estimated the model, we found "meanclarity" to be positive and statistically significant: $\beta_{meanclarity} = 0.008$, t-stat = 4.27.

14.   We also corrected for an error of omission of Broken Bow Lake in the Desvousges-Rausser model. Desvousges and Rausser claim to use "the 22 COE lakes in Oklahoma that we have data on lake levels." (D/R Report, p. 17.) But Desvousges and Rausser do not include data on Broken Bow Lake in their analysis, despite the

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[1] We coded Fort Supply *lakedepth* as 2,004 feet to be consistent with the way *lakedepth* was coded for other lakes by Desvousges and Rausser. This variable was mis-named by Desvousges and Rausser, as the data correspond to the "normal elevation at the top of the conservation pool" (http://www.swt-wc.usace.army.mil/FSUP.lakepage.html), not lake depth.

fact that the Broken Bow Lake data are available at:

http://www.vtn.iwr.usace.army.mil/recreation/reports/lake.asp?ID=54;

http://www.swt-wc.usace.army.mil/BROK.lakepage.html.

15.   If the Broken Bow Lake data are entered, as provided from the sources described
above, and the model is re-estimated, "meanclarity" is found to be positive and
statistically significant: $\beta_{meanclarity} = 0.006$, t-stat = 5.69.

16.   In addition to the separate error analyses described above, we corrected for each
combination of the three errors. We found that "meanclarity" continued to be
positive and significant for each case. When we correct for the 2007 Tenkiller Lake
visitation error and the Fort Supply Lake depth error, "meanclarity" is found to be
positive and significant: $\beta_{meanclarity} = 0.009$, t-stat = 5.35. When we correct for the
2007 Tenkiller Lake visitation error and include the Broken Bow Lake data,
"meanclarity" is found to be positive and significant: $\beta_{meanclarity} = 0.006$, t-stat = 6.13.
When we correct for the Fort Supply Lake lake depth error and include the Broken
Bow Lake data, "meanclarity" is found to be positive and significant: $\beta_{meanclarity} =$
0.008, t-stat = 8.12. When we correct for all three errors, i.e., the 2007 Tenkiller
Lake visitation error, the Fort Supply Lake depth error, and include the Broken Bow
Lake data, "meanclarity" is found to be positive and significant: $\beta_{meanclarity} = 0.008$, t-
stat = 8.87.

17.   We have concluded that when any, all, or any combination of the coding errors and
the error of omission are corrected, the indicator of water clarity that Desvousges
and Rausser rely upon, "meanclarity," significantly predicts lake visitation. This
directly contradicts their claim.

DESVOUSGES AND RAUSSER'S "TURNBULL" APPROACH IS BASED

ON A FLAWED METHODOLOGY

18.   In the D/R Report, Desvousges and Rausser claim that the "Turnbull" estimator is a more conservative approach to estimating willingness to pay ("WTP") than the ABERS estimator, which is the estimator used in the CV Report (D/R Report, p. 91). This assertion is false. The Turnbull estimator and the ABERS estimator give identical results for the type of data considered in the CV Report: single-bounded data, with "yes" and "no" votes in response to a single vote question.

19.   In the D/R report, Desvousges and Rausser incorrectly estimate the "Turnbull" estimator, which leads them to their flawed conclusion that the "Turnbull" estimator is more conservative than the ABERS estimator. Desvousges and Rausser's incorrect calculation reflects a lack of research into the peer-reviewed literature, as well as their failure to check the math in the one non-peer-reviewed reference they cite to support their estimator (Haab, T.C. and K.E. McConnell, 2002, *Valuing Environmental and Natural Resources,* Edward Elgar, Cheltenham, U.K.). Their failure to check the math leads them to a flawed conclusion regarding how to deal with "non-monotonicity" in the data, which leads them to the flawed conclusion that there is a difference between the two estimators when there is a non-monotonicity. There is no difference between the two estimators for the case of single-bounded data, whether or not a non-monotonicity occurs.

20.   The literature on the Turnbull estimator includes, at the very least, the following two peer-reviewed articles: Turnbull, B., 1974, "Nonparametric Estimation of a Survivorship Function with Doubly Censored Data," *Journal of the American*

Statistical Association, 69:345, 169-173; and Turnbull, B., 1976, "The Empirical

Distribution Function with Arbitrarily Grouped, Censored and Truncated Data,"

Journal of the Royal Statistical Society, Series B, 38:3, 290-295.

21.   The acronym ABERS comes from the complete author list of the following peer-

reviewed article: Ayer, M., H.D. Brunk, G.M. Ewing, W.T. Reid, and E. Silverman,

1955, "An Empirical Distribution Function for Sampling with Incomplete

Information," *Annals of Mathematical Statistics*, 26, 641-647. This article

establishes the estimation procedure now well-recognized in the statistical literature

as the ABERS estimator.

22.   Desvousges and Rausser did not provide any of the above-mentioned articles in their

considered materials.

23.   In their article, Ayer et al. show that their ABERS estimator is the nonparametric

maximum likelihood estimator under a monotonicity constraint for the case of

single-bounded data, which is the type of data the Stratus team collected. Maximum

likelihood estimation is the standard approach to estimation in the field of

economics.

24.   The Turnbull estimator, as defined in the above-mentioned articles, is an extension

of the ABERS estimator to the case when data are "doubly-censored," also referred

to as "double-bounded." The data in the CV Report are not double-bounded. The

Turnbull extension to the ABERS is therefore irrelevant to the type of data in the CV

Report.

25.   In fact, Turnbull explicitly states that the ABERS estimator is the maximum

likelihood estimator when the data are single-bounded. Specifically, Turnbull states:

"Finally, there is the special case of all $\delta_i = 0$ [the case where there are no double-bounded observations, only single-bounded]. Ayer et al. [1] have derived explicit expressions for the maximum likelihood estimates..." (Turnbull, B., 1974, p. 170.)

26.   Desvousges and Rausser incorrectly claim that the "Turnbull" estimator of WTP is different from the ABERS estimator when the data collected do not exhibit monotonicity across bids. Monotonicity, in the context of the type of voting data presented in the CV Report, means that votes in support of a program should consistently go in one direction -- by economic theory, down -- as bids (the cost to households of the program) increase. Due to sampling (the use of a sample to represent a population), pure monotonicity is not always manifested in collected data. In the case of the data presented in the CV Report, there is a modest non-monotonicity between the bids of $80 and $125. Respondents voted "yes" 60.2% of the time to the bid of $80 and 61.5% of the time to the bid of $125.

27.   When non-monotonicity occurs in the data, the ABERS solution is to pool the two relevant percentages and apply the average of the percentages (weighted by their respective sample sizes) to both bids. The ABERS estimator therefore estimates WTP based on an averaged response of 60.9% to both the $80 and the $125 bids.

28.   Desvousges and Rausser, basing their assertion on an excerpt of a non-peer-reviewed book (Haab and McConnell, 2002), claim that when there is a non-monotonicity, the "Turnbull" estimator pools the responses but applies the pooled result of 60.9% only to the $80 bid. They claim that the $125 should be dropped from estimation. The effect of their claim is that the $125 bid is assigned the response to the $205 bid. This response is 43.5%. The Desvousges and Rausser estimator, therefore, explicitly

reduces the vote probability at the $125 bid from 60.9% to 43.5%, which is a 17.4% reduction. There is no basis for this reduction. It is arbitrary and does not come out of any peer-reviewed statistical, theoretical, or mathematical derivation.

29. While Turnbull does not explicitly discuss monotonicity in his articles, it can be shown mathematically that each iteration of the Turnbull approach would produce results that are consistent with the monotonicity constraint. (Day, Brett, 2007, "Distribution-free Estimation with Interval-Censored Contingent Valuation Data: Troubles with Turnbull?" CSERGE Working Paper EDM 05-07, also published under the same title in 2007, *Environmental and Resource Economics,* 37:4, 777-795.) In other words, the monotonicity constraint is incorporated into the mathematical solution of the Turnbull estimator. It is not an external "rule" that must be imposed, as Dr. Rausser implies in his deposition testimony (Rausser 5/13/09 Depo Tr., pp. 146, line 20 to 147, line 5). Any such "rule" would be an arbitrary decision on the part of the researcher that is neither supported by the peer-reviewed literature nor by any peer-reviewed statistical, economic, or mathematical theory. The Desvousges and Rausser approach of dropping the $125 bid is an example of such an arbitrary decision.

30. The one reference Desvousges and Rausser provide in their considered materials in support of their interpretation of the "Turnbull" estimator is a few pages from a non-peer-reviewed book chapter by Haab and McConnell (2002), a reference that Dr. Rausser described as a "perfect substitute" for the peer-reviewed literature (Rausser 5/13/09 Depo Tr., p. 146, line 7). But the pages from the chapter that Desvousges

and Rausser produced in their considered materials do not represent the complete discussion of the Turnbull approach that Haab and McConnell present.

31.   The section in the Haab and McConnell book chapter that was not included in the Desvousges and Rausser considered materials presents a mathematical proof. This is contrary to Dr. Rausser's claim in his deposition that they do not offer a proof. (Rausser 5/13/09 Depo Tr., p. 147, lines 8-10: "I already explained it. They don't offer a proof.") The proof provided by Haab and McConnell is a maximum likelihood derivation under a monotonicity constraint, which is the ABERS approach described above.

32.   The Haab and McConnell proof is the basis for the pages that Desvousges and Rausser rely upon, but Haab and McConnell commit a number of mathematical errors in their proof. First, Haab and McConnell do not explicitly solve the Kuhn-Tucker conditions for their maximization problem under a constraint. The Kuhn-Tucker conditions are the mathematical equations that must be satisfied in order to obtain a solution to the maximization problem under the constraint. Haab and McConnell do not show mathematically that they have satisfied these conditions. Second, later in the proof, Haab and McConnell draw a flawed generalization from one solution. Specifically, they solve for one particular probability density function, f_1^*, and then generalize from that solution to all t_j and t_{j+1}, where the j subscript would represent any bid. This generalization is not supported by the math, which leads Haab and McConnell to commit their third error, which is the statement that the $(j+1)^{th}$ price should be dropped. They do not explain how they derive this result, and in fact, the statement is not supported by the math (see the following paragraph).

Fourth, equation 3.16 is not explicitly derived and the values over which the summation is taken do not appear to be correct. (Haab and McConnell, 2002, p. 69.)

33.   In addition to the above-mentioned errors, Haab and McConnell commit another error. On page 69 of their book, they assume that a non-monotonicity occurs at "bid2." The Kuhn-Tucker solution, they say (but do not prove), is to set the probability density function ("pdf") to zero at that bid amount: $f2 = 0$. Where Haab and McConnell commit their error is in not recognizing that the cumulative density function ("cdf"), F2, is still defined. As they state in Table 3.3 on page 66, the cdf is the sum of the pdf's to that point. In the case of "bid2," the cdf, $F2 = f1 + f2$. But, as assumed above, $f2 = 0$, so that $F2 = f1$. Knowing that $F1 = f1$ (also from Table 3.3) gives the result that the cdf's at bid1 and bid2 are equal: $F2 = F1$. In other words, the cdf is flat between bid1 and bid2 (between \$5 and \$10 in the numerical example that Desvousges and Rausser consider); the cdf is the same at both bids.

34.   What the math above implies is that, rather than dropping the higher bid when there is a non-monotonicity, the "pooled" probabilities, as Haab and McConnell call them, should be applied to both bids. In other words, in the Haab and McConnell numerical example (Haab and McConnell, 2002, p. 77), the \$10 bid should not be dropped as Desvousges and Rausser assume. Instead, the probability calculated by pooling the \$5 and \$10 bids in the Haab and McConnell example should be applied to both bids. This is exactly how the ABERS estimator is calculated. What the math that Haab and McConnell provide shows (after correcting for their errors) is that the Turnbull and ABERS approaches are identical for the case of single-bounded data, the type of data considered in the CV Report.

35.   In his deposition testimony, Dr. Rausser was asked if any researchers estimate the "Turnbull" as Haab and McConnell describe in their book (Rausser 5/13/09 Depo Tr., p. 144, lines 1-5). Dr. Rausser answered that Haab and McConnell do (Rausser 5/13/09 Depo Tr., p. 144, line 6). But in the peer-reviewed article by Haab and McConnell on this subject (which Haab and McConnell cite in their book), Haab and McConnell estimate WTP using the ABERS approach. (Haab, T.C. and K.E. McConnell, 1997, "Referendum Models and Negative Willingness to Pay: Alternative Solutions," *Journal of Environmental Economics and Management*, 32, 251-70.) In fact, they estimate this WTP using the exact same dataset that they use in their book, except that, instead of applying the lower bid of $5 that they apply in their book for the non-monotonicity region, they correctly apply the upper bid of $10. This is the ABERS approach.

36.   In addition, the Desvousges and Rausser "Turnbull" estimates in the D/R Report exhibit erratic properties that render them unreliable. For example, in Section 5.2, Table 5.2 of the D/R Report, Desvousges and Rausser find that the WTP of "active users" is less than the WTP of "passive users." This contradicts theoretical expectations. Further, and more fundamentally, the estimates for active and passive users do not average to the full sample estimate. Specifically, D/R provide "Turnbull" estimates of $135.00 for active users and $142.08 for passive users. Yet, their "Turnbull" WTP estimate for the full sample (active plus passive users) is $176.78. This makes no mathematical sense. It is a basic mathematical fact that two sub-sample means should average (when weighted to account for their respective

populations) to the full-sample mean when the two sub-samples comprise the full population.

37. Contrary to the peer-reviewed Turnbull and ABERS articles and due to the flawed mathematical derivation in the one non-peer-reviewed reference that they cite, Desvousges and Rausser incorrectly conclude that their version of the Turnbull estimator is a more conservative and more appropriate approach to estimating WTP than the ABERS estimator is. But as Turnbull himself has stated, and as the Haab and McConnell math show (after correcting for their errors), the two estimators are actually the same for the case of single-bounded data, which is the type of data considered in the CV Report. This completely contradicts their claim.

ELASTICITY ESTIMATES ARE UNRELIABLE

38. In Section 5.3 of the D/R Report, Desvousges and Rausser estimate income and price elasticities of WTP and claim that their results "raise serious questions about the validity of the Stratus CV study" (D/R Report, p. 103). However, Desvousges and Rausser fail to support their approaches to estimating elasticities with relevant references to the peer-reviewed literature. They also commit a number of errors, fail to report confidence intervals, and appear to have not understood the coding of a key variable in their analysis, the income variable. This final issue results in their dropping a large number of observations from their analysis, which can bias results and render them statistically less precise.

39. In economics, elasticity is measured as the percentage change in quantity demanded with respect to a percentage change in price. However, with WTP data, there is generally no continuous variable available to represent the quantity demanded in the

standard elasticity formula. Desvousges and Rausser attempt to estimate elasticities in Section 5.3 of the D/R Report by substituting the change in voting yes for quantity. They claim, "This technique has been repeatedly recognized in the literature." Desvousges and Rausser list only two citations to support this statement, and neither citation has anything to do with estimating elasticities (D/R Report, p. 99, footnote 64).

40. The Desvousges and Rausser approach to estimating income elasticity is particularly problematic. To check the results in Table 5.5 of the D/R Report, Mr. Horsch and I re-ran their computer code using the same dataset that Desvousges and Rausser used (DesVousges-Rausser000977-elasticities.do; DesVousges-Rausser000983-mergeddata.dta) and identified a number of errors, which are described below.

41. All of the elasticities are incorrectly reported in Table 5.5. The elasticity that Desvousges and Rausser report for the *highest* income group within quartiles, quintiles, and sextiles is actually for the *lowest* income group, and vice versa. In other words, all three sets of results are presented in incorrect, reverse order.

42. The column labeled "mean income" incorrectly reports mean income. According to a correction that Dr. Rausser offered in his deposition testimony, after being asked and checking with his staff during the break, the column, in fact, represents the mid-point of the income range for each subgroup rather than the mean income (Rausser 5/13/09 Depo Tr., p. 136, lines 12-14).

43. Desvousges and Rausser do not report the sample sizes of the income groups, which end up being as small as 115 observations for the case of sextiles. With sample sizes this small, it turns out that the estimated logit models do not have significant

coefficients. This makes the models – and the elasticities estimated based on them -- statistically unreliable.

44. Desvousges and Rausser commit a fundamental error of analysis by simultaneously disaggregating the sample into four to six income groups and then re-estimating the Stratus logit model using log-income as a coefficient. By definition, when a researcher has disaggregated the sample based on a particular variable, that variable will have little variation within each of the sub-samples. Thus, by definition, within each group, income will appear to exert little influence on the outcome. The variation will be even less when the variable is logged, as it is in the models estimated. Because of this lack of variation, the income coefficient in the model estimated will not be a statistically reliable indicator of how income affects voting and therefore, the coefficient cannot be used to estimate a reliable income elasticity.

45. Desvousges and Rausser fail to report confidence intervals or standard errors on their calculated elasticities, so there is no evidence that any of their results are statistically significant, nor is there any way to compare results via statistical testing.

46. When asked about the lack of reported confidence intervals in a different table in the D/R Report, Dr. Rausser responded that the standard errors (and therefore the confidence intervals) are "not computable." The reason they are not computable, he said, was that some of the sub-samples of the data did not have 2 primary sampling units ("psu's") per strata. Without 2 psu's per strata, he claimed they could not apply the jackknife procedure for calculating standard errors that is used in the CV Report. (Rausser 5/13/09 Depo Tr., p. 47, lines 13-22.) This statement is incorrect.

47.   The fact is that Desvousges and Rausser could have easily computed standard errors and confidence intervals in Table 5.5 and elsewhere in their report using the jackknife procedure. When a researcher does not have the two psu's per strata that are necessary to do so, he or she simply needs to collapse strata until there are two psu's per strata. In fact, the Stratus team did exactly this when their final dataset was assembled. This is documented in the CV Report (CV Report, Appendix C, "Base Survey Administration Report," Appendix A.4.1, Section 5 and Table 3). There is, therefore, no reason for Desvousges and Rausser's failure to report confidence intervals in Table 5.5 or in any other table in the D/R Report.

48.   In addition to the errors in Table 5.5, Desvousges and Rausser fail to understand the coding of the income variable. In footnote 65 on page 100 of the D/R Report, Desvousges and Rausser state, "Those respondents with coded incomes at or over $99,999,999,998 are dropped from this analysis." Desvousges and Rausser apparently failed to read the coding information that Stratus provided in their considered materials (Stratus Expert Materials Production, F:\2009-01-02 - Stratus Materials Production\Stratus\Shared Database\Main\Data.From.Westat\Deliverable_12_FINAL_2\Documentation\OKWa terExtFinalWithWeights.pdf). Codes at or above a value of 99999999998 were used to identify respondents who either did not know their incomes or refused to answer the income question.

49.   In his deposition testimony, Dr. Rausser was asked about this footnote and he also failed to recognize that these values were used in this way (Rausser 5/13/09 Depo Tr., pp. 123, line 8 to 124, line 12). Even after checking with his staff during a break

in the deposition, Dr. Rausser still failed to understand the coding: "that footnote you asked me about on Page 100 is in fact correct. There was some coding from Stratus that indicated that income levels were above 99 billion or whatever it is." (Rausser 5/13/09 Depo Tr., p. 137, lines 9-12.)

50.   Desvousges and Rausser dropped 254 observations, or 23% of the base sample, from their analysis due to their misunderstanding of the coding and their failure to impute income for the missing values (Rausser 5/13/09 Depo Tr., pp. 124, line 20 to 125, line 10), as was done in the CV Report (CV Report, Appendix E). In his deposition testimony, Dr. Rausser claimed that "a very small number" of observations were dropped (Rausser 5/13/09 Depo Tr., p. 125, line 13). 23% is, in fact, a very large percentage of the sample to drop and doing so can result in an unrepresentative sample that can bias results, and a reduced sample size that makes estimation less statistically precise.

THE CV REPORT PASSES DESVOUSGES-RAUSSER SCOPE TEST

51.   In Section 4.4.1 of the D/R Report, Desvousges and Rausser argue that the survey in the CV Report does not pass a revised scope test that they develop. Specifically, they state: "With a larger standard error, the WTP results are no longer statistically different and the scope survey cannot be used to validate the results of the base survey as required by professional standards." (D/R Report, p. 70.) But the scope test that Desvousges and Rausser develop is not supported by any peer-reviewed literature and is not conducted via a proper statistical test. When a proper statistical test is conducted, the survey in the CV Report does pass the Desvousges-Rausser revised scope test. This directly contradicts their claim.

52.   Under the Desvousges-Rausser revised scope test, the sample size used in the CV

Report is artificially reduced for the base instrument from 1,093 to 544. The

rationale behind their revised approach is that "a large enough sample size can make

any difference statistically significant." (D/R Report, p. 69). After artificially

reducing the sample size (by dropping a random selection of the observations),

Desvousges and Rausser estimate WTP for the base and scope instruments and

compare the estimated confidence intervals. Their WTP estimate for the base

instrument is $184.55 with a 95% confidence interval of $162.32 to $206.77, and for

the scope instrument, it is $138.51 with a 95% confidence interval of $112.69 to

$164.32. Desvousges and Rausser find a $2 overlap between these two confidence

intervals and conclude that the survey does not pass their scope test.

53.   Comparing confidence intervals, however, is not a proper statistical test. Dr.

Desvousges confirmed this in his deposition testimony. When asked if comparing

confidence intervals was a proper statistical test, Desvousges replied, "No, it's not a

statistical test." (Desvousges Depo Tr., p. 134, line 12.)

54.   The proper statistical test would be a t-test for the comparison of two sample means.

I estimated the t-test using the estimates and standard errors that Desvousges and

Rausser report in Table 4.7 (D/R Report, p. 71). I first had to correct for their

reporting errors. Specifically, in column 4 of Table 4.7, Desvousges and Rausser

report standard errors of 11.34 for both the base and scope versions and these values

did not appear to be correct, given the reported confidence intervals. I inferred from

the reported confidence intervals that 11.34 was the correct value for the base

version.[2] Assuming that the confidence intervals were accurately reported for the scope version, I calculated the standard error to be 13.17.[3] The t-test based on these values is 2.65.[4]

55.   Applying the same critical value of 1.96 that Desvousges and Rausser used gives the result that the null hypothesis is rejected and the scope test is passed. This directly contradicts Desvousges and Rausser's claim.

I declare under penalty of perjury under the laws of the United States of America that the foregoing is true and correct.

<div align="center">

Executed on June 19, 2009

Barbara Kanninen, Ph.D.

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[2] Assuming a critical value of 95%, the confidence interval for the base version would be $184.55 \pm 1.96(11.34) = $162.32 - $206.77, as reported in the table.

[3] To get the correct standard error for the scope version, I followed the same logic as in footnote 2, but treated the standard error as unknown: $se = (\$164.32 - 138.51)/1.96 = 13.17$. It should be noted that the true standard error would be a bit different due to the fact that the data are from a weighted and clustered sample. The CV Report follows the standard statistical practices for dealing with these complications; DR ignore them. In his deposition testimony, Dr. Rausser confirmed that the standard error in question should have been "approximately 13.2." (Rausser 5/13/09 Depo Tr., pp. 85, line 23 to 86, line 4).

[4] $t = (184.55 - 138.51)/(11.34^2 + 13.17^2)^{1/2} = 2.65$.

<div align="center">

19 of 19

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